

A

NON-MEDICAL ESSAY

ON

SPASMODIC CHOLERA.

BY

PHILO-MEDICUS.

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TO

DOCTOR JOSHUA HARVEY,

of Dublin,

WHOSE LIBERAL AND DISCRIMINATING MIND

WILL

VALUE IT AT WHAT IT IS WORTH,

THIS

UNPROFESSIONAL ESSAY

IS

MOST HUMBL Y INSCRIBED.



P R E F A C E.

THE subject of the present Essay having claimed the Author's attention, in common with that of all mankind, wherever it appeared, it is not surprising that it should have become with many, after it had passed, a subject of inquiry and reflection, as it had been, during its prevalence, of astonishment and alarm.

It was so with the writer, and with such feelings he ventured to put his observations upon paper ; but the Medical "Trade" having objected to it, as not having a Medical Accoucheur to usher it into the world, he threw it by, till the following paragraph, in a Dublin newspaper, again recalled his attention to the subject.

From the Dublin General Advertiser of July 6th
1850.

"OXYGEN GAS A CURE FOR CHOLERA—Doctor Macrae, civil surgeon at Howrah, has, according to the *Indian Times*, discovered a new and most successful mode of treating Cholera patients. He causes them to inhale a certain portion of oxygen gas, which

causes a strong stimulus to the frame, and finally throws the patient into a refreshing sleep. On awakening he finds himself restored to health, with the exception of the general weakness which always succeeds any physical prostration. Doctor Macrae has tested his mode of practice upon fifteen European seamen, who had been carried to the Howrah Hospital in the last stage of the disease, and the patient has in every instance recovered."—*Allen's Indian Mail*.

How the "discovery" was hit on, we are not told. Of the "rationale," of the matter we are left in perfect ignorance. In the present Essay we have assigned our reasons for every process suggested, and the motives for the prominent place given to the pure oxygen of the atmosphere, as well as the application of oxygen gas, in the curative process, nor, in my apprehension, is oxygen without its beneficial effects in the free "exhibition" of cold water as a drink, as well as the cold water bath.

We now once more propose presenting the Essay to the eye of the Public, tho still declining to introduce it to their notice under borrowed colours, being really, what it professes to be,

A NON-MEDICAL ESSAY.

PREFATORY OBSERVATIONS.

PHYSIC has had its fashions as well as dress. There is scarcely a substance so filthy or obscene in nature as not to have figured in the Pharmacopias of a former day, now admitted to have had nothing to recommend them but their nauseousness.

Under the terrors of the "Faculty," it was once rendered a capital offence to burn pit coal in the city of London; and one unfortunate citizen underwent the extreme penalty of the law for his *crime*.

At another and subsequent period, they were loud in their remonstrances against the use of barm as a ferment for bread, instead of the "wholesome" leaven, which, plastered against the wall from a former baking, generated, along with *other* things, the necessary aid for raising, or leavening, the succeeding "sponge."

The next "innovation" that roused the indignation of these gentlemen, was the introduction of the denounced "hop" as an ingredient in beer, instead of the favoured "horse-aloes," or other bitters of the brewers' druggists of that day.

These times are past ; yet it is within our recollection, that a draught of pure air, or a drink of cold water, were as carefully denied to the cravings of the fever patient, as though they were the inflictions of some inimical power, that should be combatted with unceasing vigilance ; and we can also recollect the good service rendered by the ferule of a beneficent umbrella, in punching out the broad glass of the window, when the crevices of the sash were "listened," and the refreshing exultation with which we once more inhaled the breath of Heaven.

Denied the *cup* of cold water, our long unslaked thirst made amends with the *pitcher* ; and while the tottering feet could reach, or the trembling hand retain the croft, the refreshing beverage was not spared. The result was decisive. The pulse soon recovered its tone, and the stomach the well-known indications of returning health.

The system is now changed, and we have had the gratification to see rationality triumph through all our Fever Institutions ; and again I partake, by

sympathy, on behalf of their inmates, of the exultation which I myself once enjoyed in the possession of the two best medicines in nature, the crystal spring and the pure air of Heaven.

Yes! Let us hope that those times are passed; for, where humanity does not counteract professional interest, nor common sense professional prepossession, it will still be found, that the physician is as much the slave of that feeling, emphatically characterised as that of "the shop," as the veriest huxter in the metropolis.

Is it necessary that I should adduce proofs? I will do so. The immortal Harvey stated to Aubrey, that after his book on the "Circulation of the Blood" came out, "he fell mightily in his practice;" that "it was believed by the vulgar he was crackbrained," and, that "*all the physicians were against him.*" Aubrey himself says, "I knew several *practitioners* who would not have given three-pence for one of his 'bills,' that is, his prescriptions, and who said, that a man could hardly tell, by his bills, what he did aim at." In fine, his brethren of the Faculty, of the "high vulgar," avenged themselves of his superiority, by uniting with the low vulgar, in conferring on him, as by common consent, the nickname of "The Circulator." Well may the writer of his life, prefixed

to his works, as published by the Sydenham Society, exclaim: "So has it ever been with those who have added to the sum of human knowledge!"

The same feeling has descended still nearer to our own time. The great Jenner, long after the benefits of vaccination were universally admitted and practiced, was *refused a license to follow his profession!*

There was not the plea of novelty here to extenuate the refusal. Yet, even if there were, we would be inclined to exclaim with Pope,

"I lose my patience, and I own it too,

When 'things' are censured, not as bad, but new."

or, more sily, describe the man indulging in such feelings as one

"Qui redit ad fastos, et virtutem estimat annis,

Miratusque nihil, nisi quod Libitina sacravit."

But to revert to the immediate subject of this Essay. From whatever source it has proceeded, the discrepancy of opinion, as to the cause and treatment of Spasmodic Cholera, has been disgracefully conflicting. It has been confessed by one high medical authority, that "all hitherto advanced as to the *cause* of Cholera, is mere conjecture." By another, we are told that, "of the *remote causes* of this extraordinary malady we know nothing."

By one we are informed, that “the *modes of treatment* of Malignant Cholera are almost infinite.” By another, it is the expressed opinion, that, “if all the patients had been let alone, the mortality would have been much the same as it has been.” And again, that “if let alone, the patients would have *died much more comfortably.*”

After such confessions on the part of professional men, I do not think it can be deemed presumptuous for even the unprofessional to offer an opinion, whether as respects the *cause*, the *effects*, or the *treatment* of Spasmodic Cholera. The scourge has been removed, and thanksgivings formally offered up for its removal; but it *may*, and probably *will* return, and I shall make no apology for calling attention, during our respite, to the retrospection of a subject which has not been treated of by the Faculty, that I am aware of, in a manner creditable to themselves or which its importance might justly claim.

December 31st, 1849.



NON-MEDICAL ESSAY

ON

SPASMODIC CHOLERA.

It is now nearly eighteen years since the appearance of Spasmodic Cholera in these countries, and twenty-three since its recognition as a distinct disease at Jessore: yet I have never met with a treatise on the subject worthy the physician or the philosopher; nothing to generalize the separate views that have been entertained respecting it.

In its diagnosis, indeed, the leading symptoms were too decisive to occasion much discrepancy in the several notices we have of it, but, as respects its cause, the most dissimilar and speculative opinions have been given; and those of a therapeutic character are still more conflicting.

As to its cause, one ascribes it to galvanic, another to telluric influence—one to marsh miasma, another to a specific kind of corpuscles floating in the air, and inhaled with our breath—one makes the head the seat of the disease, another the stomach—one

ascribes it to want of elasticity in the skin, another to the want of saline matter in the blood. But if the disease be really a new one, a fresh graft on our nosology, should we not look for its origin in causes equally new, and not in those which have been in operation, at least since the deluge.

Some, however, are of opinion, that it is not a new disease, and that it was known to Hippocrates and the Greek physicians; while others assert it to be so perfectly modern as to have been wholly unknown to Sydenham and Cullen.

If not a new disease it is certainly singular that, notwithstanding our long intercourse with India, from which we admittedly derive it, that we should have remained in perfect ignorance of its existence until the period of its fatal appearance at Jessore. This cannot be accounted for from any want of competent medical practitioners there to note its peculiarities, nor of judicious and reflective minds to be struck by its character and impressed by its effects.

That it existed in India previous to the date of its appearance at Jessore, we have abundant proof. The very name by which it is designated of "Spasmodic Cholera," was given it by Mr. Curtis in 1807, who, however, regarded it then as a new disease, and his opinion is entitled to some weight.

To form a correct judgment on the subject, as to its being a new disease or otherwise, a very strict comparison of the symptoms recognised as belonging to "Spasmodic Cholera," with those symptoms re-

corded by the old physicians under the general head of "Cholera," and with those of our own physicians under those of "Bilious Cholera," would be requisite; but this is not the purpose of the present essay, and would demand an extent of research and discrimination that I cannot pretend to bring to it, and that no non-medical person should presume to do, however interested in so rational and so scientific an investigation.

To place the subject, as relates to its diagnosis, before the view, I shall transcribe the symptoms as given by a few of those who have treated of it, and some of whom, from their opportunities of observation and other qualifications, will be considered competent authorities on the matter.

Mr. Curtis states, that soon *after* the attack, "the spasms begin to affect the muscles of the thigh, abdomen, and thorax, and, lastly, passed to those of the arms, hands, and fingers."

It will be observed here, that the spasms *succeeded* the other symptoms, while, in the cases reported by many, the spasms almost invariably *preceded* them. In Mr. Curtis's enumeration of the parts affected by the spasms, he very much reverses the *order* of their occurrence, as observed by others, who represent them as *first* attacking the extremities, and so proceeding to the trunk; commencing in the toes, and advancing successively to the legs, thighs, and abdomen, and simultaneously commencing in the fingers, and advancing to the hands, the arms, and the thorax.

“The disease,” says Mr. Whyte, “commonly begins with a watery purging, unattended with griping or any pain. At an interval of, generally, from half an hour to five or six hours, and sometimes without any interval, the patient vomits a white fluid, uncombined in any instance with bile, of which there is abundant evidence in every quarter. The spasms, in the division of the army from which this description is drawn, made their attack at no determinate period of the disease, but, in general, not for many hours after the commencement of the vomiting and purging. There was soon great debility and sinking of the pulse; the extremities became cold; the eyes sunk in their sockets; the vessels of the *tunica adnata* were injected with red blood, over which, if the disease advanced, a film was formed; the features expressed the deepest anguish; and the eyelids were either wholly or half-closed. The patient invariably complained of great heat at the stomach, and *called incessantly for cold drink*, although warned of the danger attending its use.”

What the danger apprehended was, we are not informed, nor of any fatal or other result in consequence of compliance with the urgent call. But to continue :

“The tenesmus now became violent, while nothing was discharged but the fluid just mentioned, and a substance like the coagulated white of an egg. The uneasiness and jaetitation were so great, that it was with the utmost difficulty an opportunity could be

gotten of feeling the pulse, which by this time was not always perceptible, although it was generally so until the spasms came on. These were always of the rigid kind, attacking first the toes and legs, and then extending to thighs, chest, and arms. When they reached the chest, the breathing became so difficult, and the sense of suffocation so extreme, that the diaphragm most probably associated in the spasmodic action."

The order of progression of the spasms, as given by Mr. Whyte, coincides with that given by Mr. Curtis, except that the latter passes over the toes and legs, making the spasms to commence at the thighs, but both make the spasms to proceed *from* the lower extremities through the trunk *to* the upper, and therefore *down* the arms, hands, and fingers, as expressed by Mr. Curtis.

"The most unfavourable and dangerous signs in the ordinary progress of the disease were—a coldness of the surface, extending over the region of the heart and stomach. The skin under the nails became incurvated; the tongue was icy cold; a universal colliquative sweat broke forth, with a shrivelling of the palms of the hands and soles of the feet; the spasms gradually declining as these symptoms increased. In general all pain and spasm left the patient before death; and even when the heart could not be felt to beat, he expressed himself easy, and said he was better. Sometimes, however, he was at this period in the greatest agony, rolling himself on

the ground, groaning, and even bellowing most pitcously; signs chiefly occurring in patients who lingered three or four days before death came to their relief."

The following appearances are stated to be "worthy of notice" on dissection: "An enormous distension of the stomach and bowels, not from air, but a gelatinous substance; little sanguineous turgescences on the surface of the organs, but an absence of the *moisture* and glossy character of health; the liver much enlarged from the quantity of blood contained in its vessels, and, on one part of its convex surface, a considerable extravasation of blood; the gall-bladder filled with bile, and projecting beyond the edge of the liver; the bile of a very dark colour, and the gall-ducts pervious. The contents of the small intestines were dark coloured, apparently from an admixture of bile; the contents of the large intestines resembled the white albuminous matter that was discharged before death. The urinary bladder was quite empty and wholly shrunk into the pelvis; the kidneys apparently diminished; the lungs so much collapsed as hardly to fill one-half the cavity of the chest; and no fluid in the pericardium. Of the breast itself, or the brain, we have no account."

These appearances are, as stated, justly worthy of notice; but what *is* remarkable in them? Not the distortion of the stomach or bowels by a gelatinous substance, for this was what might have been expected; nor the state of the liver or gall-ducts, for

this might have been anticipated. The circumstance worthy of notice is contained in these words, "The contents of the small intestines were dark coloured, apparently from an admixture of bile." If this were not bile, what was it? If it were, the patients were very unfortunate; for in every instance where the bile made its appearance, we are told elsewhere the case turned out favourably.

"Such were the appearances in the body of a Sepoy. In the European subject they were the same, with the two following exceptions: The stomach and intestines in the European were distended with wind, instead of with the gelatinous fluid, and hence collapsed upon puncturing them: the veins on the outer surface of both, as well as of the meso-colon, were tinged with blood."

Here are facts indeed worthy of notice, but have we no attempt at accounting for them? Why should the intestines of the Sepoy be distended with the undischarged Cholera fluid, and that of the European be distended with wind only? Were these general cases, or merely insulated ones? If general, I would expect some offer at accounting for the circumstance, which is too remarkable not to merit some attempt at investigation.

We have here also an additional fact stated not to be passed over as undeserving attention * * * *
"The disease proved every where more fatal to natives than to Europeans: and, among the former, no blood, in numerous instances, could be drawn from the arm, however urgent the symptoms."

Are we to suppose the idiosyncrasy of the Sepoy altogether different from that of the European. From the above extract it would appear that the latter yielded readily to the lancet, in India. Has the same fact been observed at home?

The Bombay accounts are stated to differ only in a few particulars from the above. "In a large proportion," says Mr. Orton, "there is no appearance of spasm in any part of the system. In many there is no purging, in some no vomiting: and in others neither of those symptoms. I have already observed, that these last were by far the most dangerous cases, and that the patients died under them, often in an hour or two, the nervous power appearing to be exhausted almost instantaneously, like the electric fluid from a Leyden jar."

So here we have Cholera without any flow from the intestines, and Spasmodic Cholera without spasms. These are contradictions in terms, only to be reconciled by the supposition, that these distinctive characteristics of Spasmodic Cholera were present, but were anticipated in their effects by another and more fatal one. Were it otherwise, it were high time the name should be altogether changed and a more suitable designation given it. What the state of the stomach and bowels was, under such circumstances, I am not aware; but should confidently look to their supporting the usual phenomena, by exhibiting the blood in that state of dissolution inseparable, as I would say, from the disease; and the suspension

of the excretory functions of the liver and other viscera.

But Mr. Orton again states, that in some cases there was an overflow of yellow bile, though in excess only in the milder cases. This is still in opposition to the recognized characteristics of Spasmodic Cholera, and lessens materially the space between the lines of demarcation assigned to Spasmodic Cholera, as contrasted with the symptoms of general Cholera, by Cullen, by Sydenham, and even by the Greek physicians.

Now, what is Sydenham's description of Cholera.

"Vehement vomitings and difficult and painful dejections of *ill conditioned* fluids: agony, and inflammation of the intestines and abdomen; cardialgia; thirst; a quick pulse, often small and unequal; heat and anxiety; nausea and colliquative sweat; spasms of the arms and legs; fainting; coldness of the extremities, and other symptoms of equal danger, which terrify the by-standers and kill the patient in twenty-four hours."

In how many particulars does this coincide with the diagnosis of Spasmodic Cholera! But we have still further to add—Some of the cases that occurred to Sydenham, in Bilious Cholera, were so rapidly fatal that, as is stated, "this distinguished pathologist, has also been conceived to have been acquainted with the present species," that is, with Spasmodic Cholera, "and to have included it under them," that is, under the symptoms of Common or Bilious Cholera. Again,

Dr. Cullen, who is stated by Mr. Good to have loosely copied Sydenham's remark—"that Cholera is sometimes so severe in its symptoms as to destroy life in twenty-four hours," is yet, by the same authority, represented as regarding it in no species as a serious complaint, "and prescribing nothing more for its cure than "a plentiful exhibition of mild diluents."

There is some ineongruity here. How is it possible the great Cullen could adopt the idea, and the language of the great Sydenham, in regarding a disease as "in no species a serious one" which yet "appalled the by-standers and killed the patient in twenty-four hours."

Celsus' diagnosis of Cholera can by no means be regarded as "a more minute description of the Cholera of Sydenham." The language of Celsus is clear and unmistakeable, as referring to Bilious Cholera alone, "*Bilis supra infraque erumpit, primum aquæ similis, deinde ut in eâ recens earo lota esse videatur, interdum alba, nonnunquam nigra, vel varia*"—what has the "*Bilis supra infraque*" to do with the vomitings or dejections of "ill-conditioned fluids," which may be of a very different character. We may infer them to be the same, but only in the conviction of Spasmodic Cholera being unknown to Sydenham, and thus excusing the want of precision in the loose term, of "ill-conditioned fluids," which would be unpardonable if the existence of both diseases at one and the same period had rendered greater precision necessary. I cannot however but incline to the opinion, that

Spasmodic Cholera, *as an Epidemic*, is a new disease; and that the insulated cases, of at all a similar character, noticed by the old physicians, form the exception not the rule. It would be extraordinary indeed if no case approaching to the character of the late epidemic had ever before occurred in the course of three thousand years.

Without entering on the question at present, I shall place the leading features of the disease as drawn by different pathologists before the judicious reader, who can thus scarcely fail to form a correct conception of the original, and thus tempt some ingenious enquirer to investigate a subject involving such deeply important considerations, and such very extraordinary phenomena as this disease exhibits.

The following account published by the London Board of Health, gives a very graphic sketch of Spasmodic Cholera, as it appeared in India.

“The attack of the disease in extreme cases is so sudden, that, from a state of apparent good health, or with the feeling of only trifling ailment, an individual sustains as rapid a loss of bodily power as if he were suddenly struck down, or placed under the immediate effects of some *poison*; the countenance assuming a death-like appearance, the skin becoming cold, and giving to the hand, as expressed by some observers, a sensation of coldness and moisture such as is perceived on touching a frog; or, as the coldness of the skin of a person already dead. The pulse is either feeble, intermitting, fluttering, or lost; a

livid circle is observed round the eyelids: the eyes are sunk in their sockets; the tongue is cold, and, either clean, or covered with a slight white fur; and, in many instances even the breath is cold. In cases of this severity, the vomiting and purging, characteristic of the disease, do not commonly take place so early as in milder attacks, but seem to be delayed until the almost overpowered functions make a slight effort at re-action. It is worthy of remark, that, unless death takes place in these extreme cases within a few hours, some effort of the animal powers is made to rally the constitution; and this point is insisted on here, because it will direct the mind to the particular moment, when bleeding and certain other parts of practice, recommended in the India Reports can be enforced in this country with probable success. Vomiting soon succeeds; first, of some of the usual contents of the stomach; next, of a turbid fluid-like whey, white of egg, water-gruel, or rice-water; described perhaps more accurately, as a serous fluid containing flocculi of coagulated albumen. The lower bowels seem to let go their contents; what happens to be lodged in the rectum is passed more or less in its natural state; the next discharges are similar to those thrown up from the stomach, and are passed with violence, as if squirted with a syringe. The same similitude may be applied to the vomiting. Spasms, beginning at the toes and fingers soon follow, and extend by degrees to the larger muscles of the legs and arms, and to those of the abdomen. These

vary in intensity, but are sometimes so violent as to put on the appearance of tetanus."

"In some severe cases the vomiting is slight, in others considerable; and the purging and vomiting precede each other without any known rule; but which ever may be the precursor, a *burning heat* is early felt at the precordia; there is an *invincible desire for cold liquids, particularly water*; and although the skin and tongue are cold to the touch, and the pulse nearly lost, or altogether imperceptible, the patient complains of *intense heat*, and *has an almost insuperable aversion to any application of it to the skin*. The spasms increase, sometimes spreading gradually, sometimes suddenly to the abdomen, as high as to the 'scorbieulus eordis.' The next severe symptoms are, an intolerable sense of weight and constriction felt upon the chest, accompanied by anxious breathing, the spasms continuing at the same time; a leaden or bluish appearance of the countenance, the tongue, fingers, and toes, assuming the same colour; the palms of the hands, and soles of the feet becoming shrivelled; the fingers and toes giving the appearance of having been corrugated by long immersion in hot water. There is, throughout, a suppression of the secretion of urine, and of the secretions of the mouth and nose; no bile is seen in the evacuations, and it may be generally observed, that, all the functions employed in carrying on life are suspended or alarmingly weakened, except that of the brain, which appears to suffer but little, the intellectual

powers usually remaining perfect to the last moment of existence."

The symptoms as they appeared, in London, are thus given by Dr. Elliotson:—

"There was a great variety as to the mode of attack; and as to the order of the symptoms after they had begun. With regard to the first point, some persons were attacked suddenly, while others had diarrhœa for some time previously. Those who were suddenly attacked were generally seized early in the morning, after going to bed perfectly well; and discharged, by vomiting and purging, a turbid whitish fluid containing white flakes. There was violent pain in the abdomen; violent pain in the extremities; and dreadful cramps in the fingers, toes, arms, and legs. In two or three hours from the moment of attack, I have seen the eyes sunk in the sockets; blueness round the base of the eyes; blueness of the nails; in some cases, blueness of the extremities; in one case blueness of the whole body; no discharge of urine; *intense thirst*; *a great sensation of heat* within; extreme restlessness; the tongue of a colour like lead, and, in the case in which the body was blue all over, covered with a white and leathery crust; a very feeble pulse; and at last no pulse at the extremities, though it could be felt at the heart. The pulse and the respirations were very rapid. I have counted the respirations at thirty-six in a minute. There has been a great fall of temperature, so that the hands, the tongue, and the

breath as it came from the body, were cold. On passing a thermometer into the mouth, I have found it as low as 84° , ranging only at any rate between 80° and 90° . There has been a cold sweat on the extremities; and then, at last, the patient would remain free from pain, restlessness, or vomiting; and would lie perfectly still, conscious of everything, but with a desire not to make the least exertion; apparently tranquil and waiting for his dissolution. Then, before death, I have noticed the temperature rise, the coldness cease; and after death the temperature has risen still more. In the case where there was such great blueness, no sooner had the patient expired than the blueness was diminished. I did not observe whether it diminished before death, but I noticed it immediately afterwards; and in an hour there was nothing of the colour to be seen. There was a twitching of the muscles, so that one finger after death would be drawn in, and then another; the lower jaw would move up and down; and we might see a quivering of the muscles inside the thighs. The voice was weak, and there was a great peculiarity in it."

I shall conclude with the diagnosis of this terrible epidemic as given by Dr. Gregory, which is conclusive as to most of its leading characteristics.

"This disease sometimes commences suddenly, with weakness, trembling, giddiness, and nausea, quickly followed by vomiting and purging. At other times it is preceded for, one, two, or even three days by diarrhoea. When the malady is fully developed,

the following are the most characteristic of its symptoms:—1. The matters rejected, both upwards and downwards, have the appearance of rice-gruel, or of whey mixed with white floeculi. The vomiting and purging are unremitted, being often aggravated by food or medicine, still more by the draughts of *cold water* which *an intense thirst induces the patient urgently to demand*. 2. The whole surface, but particularly the face and extremities, assume a leaden blue or purplish tint. The fingers appear shrunk, damp, and as if long soaked in water. The respiration is hurried and laborious, and the voice indistinct. Blood drawn from the arm appears in drops of a deep black colour. 3. Spasms begin in the fingers and toes, and thence, gradually extending to the calves of the legs and fore-arms, invade in severe cases the trunk of the body, and are aggravated by any, even the slightest, exertions. 4. There is a painful restlessness; the patient tosses, about incessantly from a *sense of heat*, weight, and anguish about the preeordia. 5. The pulse gradually lessens its force until it becomes imperceptible in the wrist. The skin is cold. The tongue is cold to the touch, and a thermometer introduced under it indicates a fall of eight or ten degrees below the natural standard. 6. The countenance collapses. The eyes appear fixed and glassy, sunk in their sockets, and surrounded by dark circles. There is great prostration of bodily strength. 7. The secretion of urine is almost totally suspended. 8. Delirium is seldom present, and the patient often

breathes his last with his senses entire. An indifference to life is frequently observed through the whole course of the disease."

Its special diagnosis is compendiously summed up by the same authority in the following words.

"The chief points, cognizable during life, in which the new or epidemic form of Cholera differs from the Cholera of Sydenham are the following :—

1. In the gruel-like appearance of the evacuations.
2. In the predominance in an early stage of the disease of symptoms depending not on mere debility of the circulating powers, but on *mal-oxygenation* of the blood.
3. In its occurrence in the winter and spring seasons—the disease raged at Moscow and the neighbouring towns during the depth of winter.
4. In the phenomenon of its secondary fever.
5. In its extreme malignity and resistance to all ordinary remedies.
6. In its mode of propagation."

I may here notice two circumstances in this extraordinary disease, which all accounts coincide in attributing to it—freedom from delirium—and mal-oxygenation of the blood, characteristics that in other diseases are considered almost, if not altogether, incompatible. "Delirium is seldom present"—"the patients generally retain their intellects to the very last," is a fact stated on authority not to be questioned, and the "mal-oxygenation of the blood," is a leading, if not the leading characteristic, perhaps the sole *cause*, of all the other phenomena, of the disease. In asphyxia from other causes, the delirium and coma

are attributed to the *unoxxygenated* state of the blood impelled by the heart to the brain, and, even in Bronchitis, "the circulation of dark coloured blood" is assigned as the cause of its torpor, and "delirium and stupor" are especially stated to "indicate the effect of *unarterialized* blood circulating in the brain." Why should it not be so in Spasmodic Cholera! If like causes must produce like effects, then, either the characteristics given of this disease must be incorrect, or erroneous conclusions have been drawn from the effects in other complaints, in reference to their cause.

Two questions have been mooted, to which we shall now advert, and to which the descriptions of the disease given in the foregoing extracts, as it appears in both Asia and Europe, in India and England, will leave us little difficulty in coming to a right conclusion.

First. Is the disease, as it appeared in these countries, identical with that of India?

Notwithstanding the attempt to draw lines of distinction between them, they appear to me distinctions without a difference. Independent of the general identity of its characteristics, the affirmative is too decisively proclaimed in its progress from India to our own shores "unretarded by adverse winds," or any other obstacles that may have been opposed to it. From India it travelled to Persia, and in October 1822, as stated by Mr. Cornish, "had reached the western boundary of that Empire, and was steadily advancing in the same direction;" and the same gentleman, with instinctive sagacity, adds, what Mr.

Good terms, "the following alarming prediction." "The atmosphere is generally clear, cold and healthy; and if, in such a climate, this epidemic commits such ravages, as almost to equal its effects in many parts of India, I much fear it will extend to Europe, where the crowded cities, and great population will make it more severely felt than it has been in the scattered cities and scanty population of Persia."

This question is I think set at rest—but there are circumstances related in reference to its different effects in India, on the Natives, and on the Europeans, well deserving the consideration of the pathologist; and some of which we would not be prepared to anticipate—the various appearances presented after death—and the greater fatality to the former.

As to the second question; Is the disease a new one? The whole weight of evidence preponderates in favour of the affirmative. Yet the conclusion to which it leads, must impress us as one of a very extraordinary character: but as it appears to me an inevitable one.

If the disease be indeed a new one, then the elements of the human constitution must have been undergoing a change, of which we have had no intimation; or, the constitution of the elements themselves has undergone a change, of which we have no proof. If we admit neither of these propositions, then we must attribute the visitation to the immediate act of Omnipotence, a thunderbolt to awaken mankind from the dream of that which we call life.

Science, indeed may not very readily admit this mode of getting rid of the argument, nor should I, if I saw any other rational mode of escaping the inference.

I see but one way of solving the difficulty—the admission of the partial appearance of Spasmodic Cholera at various periods, of which we have occasional proofs, not to be questioned, but, as the formidable epidemic which it has since proved, we have nothing to support its existence, as such, at any former period ; and no carelessness of observation, or incompetency to mark its peculiar and impressive characteristics, could be adduced to account for passing over, or omitting to minutely record, the diagnosis of so extraordinary a disease.

But, even admitting this view of the matter, a combination of circumstances must have occurred to favour the development of the epidemic, and incline it to take the formidable and fatal form we have witnessed it to do ; some deduction, however, must be made from the fatality ascribed to it, by the reflection, that it is the very nature of an epidemic to attract other complaints into its vortex, incline them to assume its appearance, and elaim the merit of their triumphs as well as its own. Where a fatal epidemic prevails, other diseases generally lose their malignity ; but leave to the prevailing one only a temporary triumph, when they again assume their legitimate and recognized sway.

We have now to examine the causes assigned as accounting for this terrific scourge.

Sir James Murray attributes it to galvanic influence, and supports his theory with much ingenuity, but a very inadequate array of facts. The sudden discharge of strength in Spasmodic Cholera may well bear its illustration by the comparison introduced, "the instantaneous discharge of electricity from the Leyden jar." The theory is a rational one, but still the difficulty remains, even supposing it a correct one—What has electricity been doing these three thousand years? Why has it not during this long period produced similar effects?

Other writers on the subject ascribe it to telluric influence. This may be considered as galvanic or electric influence in a different form, or under a different modification. This idea has been supported with some talent and more confidence by Mr. Hawthorne, who brings forward strong arguments in favour of his theory, and what is better, strong facts in favour of his mode of treatment. But still the question arises—Why has this influence been now exercised for the first time?

Again, while in support of his theory, and that of non-contagion as depending on it, there are many facts adduced, among them, there is one describing the disease as "following the track of commercial intercourse," and the description has been fully confirmed by others. This *fact* contradicts his *theory*, to a certain extent at least, both as to the origin of the disease and its effects.

Some assert the brain to be the seat of the disease,

and the source from which all the other morbid symptoms are derived; but if so, is it not a little remarkable that the patients generally retain their intellects perfect to the last? I do not say the circumstances are altogether incompatible; but I do consider it improbable that they should almost invariably be the reverse.

Poisonous miasma is alleged by others to be the source of the disease, and the lungs therefore the principal seat of it. Sir James Murray makes the marsh miasma merely the vehicle of the galvanic influence. The distinction is needless. If marsh miasma be invariably found to possess certain morbid effects, it may with every propriety be put for the latent, still more for the unascertained influence, of which it is stated to be only the medium.

But of what nature can that miasma be that extends its baleful influence alike through every climate, over every character of country; and that pursues its onward track, unaffected by any and every obstacle that presents itself to retard its slow but certain, its steady but fatal and irresistible progress?

While some thus make the head the seat of the disease, and some the lungs, others assert the stomach to be the organ principally affected, and others again ascribe it to the want of 'elasticity in the skin. One attempts to account for it as proceeding from certain corpuscles floating in the air, of which a curious illustration is given in one of our amusing

periodicals; while Mr. Steevens discovers the whole source of the disease to be derived from a deficiency of saline particles in the blood.

The stomach, indeed, largely partakes of the effect of the disease; but unless we can trace it to be the organ primarily affected, we cannot consider it as the cause of the extraordinary phenomena exhibited in Spasmodic Cholera.

As to the disease depending on any want of elasticity of the skin, what proof is offered of such deficiency? Or, if such existed, how would it be possible for rationality to ascribe the instantaneous shock of the disease to a cause of which we had no intimation of the existence; and in a membrane so remote from the seat of vitality, and unsusceptible of any such terrific effect as that attributed to it.

The corpusele theory may amuse, but seems to have met no serious reception; yet, that the disease presents many symptoms analogous to those produced by inhaling certain noxious vapours cannot be denied; but we are not prepared to believe that any microscopic power can exhibit the form of the constituents of each subtle poison the breath may inhale, nor can we think the present alleged discovery has arrived at a truth that has heretofore eluded our grasp.

Similar objections exist to Mr. Steevens' theory of the want of saline particles in the blood. We cannot suppose the very various character of the constitution of the victims of Cholera to have all of them participated in this deficiency; the young, the old,

the robust, the delicate. The idea is too improbable to be entertained, and the only grounds upon which it could claim attention would be such a series of facts in favour of it, as to neutralize the improbability.

Of all the theories that have been started, those of the electric or galvanic, and the telluric have been in my opinion the best supported; and I know no other that can compete with them in probability, unless that which ascribes the disease to the inhaling by the lungs of some subtle poison, that suspends their action on the atmospheric air, and neutralizes their powers of *oxygenizing* the blood, or an *inadequate oxygenation* of the atmosphere itself. To this theory I am irresistibly impelled to incline. "The blood is the life," says the scripture, and so Physiology appears to me to say too. This life, in the disease we treat of, becomes tainted at its fountain; the atmosphere no longer affords the adequate supply; or affords it mingled with an inhaled death, that courses through all the ramifications of life, and paralyzes all their healthful action. Difficulties I admit exist even in embracing this theory, but they are such as would more than equally apply to any other, perhaps, that could be suggested, and much more to some of those which have been so confidently advocated.

The patient in Spasmodic Cholera "appears as if suddenly struck down, or placed under the immediate effects of some *poison*." The former effect may rationally be assumed to indicate galvanic or electric

influence as a cause ; the latter, the inhaling morbid matter from the atmosphere, or an insufficiency of the *oxygenous* principle, which thus excludes its life. When asphyxia takes place from an electric shock, electricity is I apprehend often resorted to in order to the restoration of the vital powers. I have never heard of its being resorted to in Spasmodic Cholera ; though apparently indicated by the practice in what is deemed analogous, by the advocates for galvanic influence as a cause. If, on the other hand, the effect is attributed to the *mal-oxydation* of the blood, the *exhibition of oxygen* in its purity would seem to be indicated, if experience did not inform us in similar cases, that, where the free application of atmospheric air did not afford the requisite supply of *oxygen* for the re-animation of the patient, no exhibition of the *pure gas* was found to avail." The matter however would be worth a trial ; the circumstances of the disease indicate it ; and the most decisive experience alone could persuade me, that injury could result from an experiment of the effects judiciously made.

Having exhibited the various opinions entertained as to the causes, as well as the several pathological sketches given of Spasmodic Cholera, we shall now take a view of the varying therapeutical systems adapted for its cure, if those can be called systems, which appear to have been governed by no certain principles, but to have been the isolated experiments

of each individual practitioner, with one or two honourable exceptions.

"The modes of treatment which have been proposed for Malignant Cholera," says one distinguished authority, "are almost infinite. Persons have recovered under each of them; but their absolute value may be deduced from the single fact, that, after being known for a period of twenty-eight years, the proportion of deaths, per cent, remains the same as when the disease first broke out at Jessore."

Here is a candid confession, that medicine, *on the average*, has done nothing for it; but very opposite deductions must be drawn from the results of certain *particular* modes of treatment, too well authenticated to be doubted; but which, in proportion to their success in lessening the fatality under such *special* modes of treatment, must increase the *average* fatality under the *general* treatment not included by it; and brand such general treatment as more fatal than even perfect neglect. This is an opprobrium on the science of medicine that should either be denied or wiped off.

Mr. Ogilvy, Secretary to the Medical Board at Bombay, expressly asserts, "that it was not ascertained that any case had recovered in which medicine had not been administered;" while among those who had received, the advantages of "the judicious and active plan concurrently pursued, the proportion of deaths was reduced to 6.6 per cent."

In other parts of India, the deaths, "under the same plan of treatment," were still fewer. "Dr. Burrell, surgeon to the 65th regiment, out of sixty cases, makes a return of only four deaths, and Mr. Crow, on the same station, asserts, that on an early application for relief, the disease, in his opinion, is not fatal in more than one in an hundred cases."

What the curative plan was that proved so successful, and which stands in such advantageous contrast to anything which we can oppose to it, unless we admit the infallibility claimed by Dr. Hawthorne for his mode of treatment, we are not left to conjecture. It is plainly stated, "The curative plan pursued with so much success consisted in—bleeding, according to the strength of the patient; calomel in free doses, of from fifteen to twenty grains in a doze; with one or two grains of opium, repeated if necessary every four, three, and, in some cases, every two hours, till the urgency of the symptoms abated: to these were added a liberal use of the most diffusible stimuli, as the spirit of nitric ether; ammonia; camphor; hot arrack and water, mixed with spices and sugar; camphor mixture; essential oil of peppermint; the *hot bath*; stimulant embrocations; and, sometimes, the antimonial powder in dozes of five grains given in conjunction with the calomel." Yet it is a little remarkable that Dr. Rankeen, of the Bengal establishment, treated both the lancet and calomel with the utmost contempt.

To what extent the most powerful medicines may,

in some cases, be exhibited, not merely with impunity but benefit, the following example will prove.

“By mistake, twenty grains of calomel and sixty minims of laudanum, were given at an interval of less than half an hour. The patient was inclined to sleep; nothing more was done, and in two hours and a half he was as well as ever he was in his life.”

Here was “a fortunate blunder,” as Mr. Good terms it, “capable of being laid hold of and applied with great practical advantage.”

I think it is a circumstance “capable of being laid hold of and applied with great practicable advantage,” by careful and judicious hands; but why are we to look to deriving a precarious advantage from “a fortunate blunder,” which should never have occurred, if we despise or neglect “the judicious and active plan” recommended by the Medical Board at Bombay, and “concurrently pursued” with such unequivocal success? A plan, not deriving its origin from a fortunate blunder, but apparently the result of enlightened reason, supported and confirmed by experience. Have we profited by its example, or can we boast its results? Let us listen to the candid statement of Dr. Elliotson.

“As respects this country, I cannot but think, that if all the patients had been let alone, the mortality would have been pretty much the same as it has been.” Again, “Some say that they have cured the disease by bleeding; others, by calomel; others,

by opium. Others, again, say that opium does harm. No doubt many poor creatures died uncomfortably, who would have died tranquilly if nothing had been done for them. Some were placed in hot water, or in hot air, and had opium, and calomel, and other stimulants, which altogether were more than their system would bear; and more than their system could have borne, if they had been so treated even in perfect health."

So much for what medical assistance has done in London generally; let us see whether the Doctor can give a more favourable account in his own particular experience.

"I am sorry to say, that of the cases I had to treat, the patients nearly all died. I tried two or three sorts of treatment. Some had opium and calomel in large and full doses; but *they died*. Hot air was applied externally; and I got two to breathe hot air. I had a tube passed through boiling water, so that they might inhale hot air. It was found vain to attempt to warm people by hot air applied externally. They were as cold as before—we could not raise their temperature; and therefore it was I thought of making them breathe hot air; but *both patients died*. It was said that saline treatment was likely to be of use, and I accordingly tried it on some patients." We are not told the result, but are left to infer, that "*they died*." Again, "In one patient, at St. Thomas's Hospital, I ordered an injection containing an ounce of the same remedy,

scequi-carbonate of soda, but the greater part of it came away, and *the patient died*.

The time, the circumstances under which the several exhibitions were made by Dr. Elliotson, we are not informed; but some of them, under any circumstances of the disease, appear to me to be irrational. "Hot air applied externally," while a furnace was burning within; and a full average portion of caloric in the body, if it could only be distributed! "Breathing hot air," while the *unoxygenized* state of the blood was calling for whatever could supply the deficiency!

Doctor Elliotson complains, that "the faculty were not in the least more informed as to the proper remedies at the last than they were when the first case of Cholera occurred."

What then becomes of the principles laid down by the Medical Board of Bombay. It does not perhaps, in every instance, direct the particular moment that each particular remedy is to be exhibited, *that* is to be left to the judgment of the practitioner, who is supposed to bring common sense to regulate his professional skill, and to apply it according to circumstances. But there is an impressive observation in the Report of this Board. "It is worthy of remark, that, unless death take place in these extreme cases within a few hours, some effort of the animal power is made to rally the constitution; and, this point is insisted on here, because it will direct the mind of practitioners to the particular

moment when bleeding and other parts of practice may be enforced with probable success."

This shows that the curative process was not one of hap hazard practice, but was adapted and acted on upon fixed and rational principles, which our home practice does not appear to have carried out, much less improved upon. I do not argue here that bleeding was always necessary, for we are plainly told it was not.

"Many of the cases proved successful without the use of the lancet : but, from a return of Dr. Burrell, the hazard of omitting it, whenever blood could be made to flow, seems rather unjustifiable : for, according to this return, out of 100 patients, 88 were bled and 12 not ; out of the former, only 2 died, being 1 to 44 ; of the latter, 8 died, being two-thirds or nearly 30 to 44."

Here is at once the admission of many cases proving successful without the lancet ; but, if the facts are to be relied on, proofs of its efficacy to an extent that renders the omission an act little short of unjustifiable homicide. I have seen no proof that we have given the Bombay Therapeutics a fair trial, and I am convinced we have pursued none that have proved more successful.

But however judicious the plan recommended by the Medical Board of Bombay, or however successful the treatment in the hands of Dr. Burrell ; even in India it does not appear to have been generally adopted. Dr. Rankeen who was on the Bengal station

is stated to have treated both the lancet and ealomel with contempt, and to have depended exclusively, from the first, upon large doses of opium and highly pungent and diffusible stimuli; a treatment approaching that of Dr. Hawthorne, and for which he claims the merit of infallibility; but I am not aware of any actual return of the eases by Dr. Rankeen, to enable us to institute a comparison with the very satisfactory return, afforded by the statements of Dr. Burrell.

At home, many have adopted the curative process of the latter, so far as free doses of ealomel, opium, and stimulants are concerned, but the practice has been uncertain and fluctuating, and the lancet has been very sparingly employed.

It is to be regretted that, of the various curative plans pursued, we have a great paucity of information, either as to the process or its results, and even where they are professedly given, the relation is marked often by an utter want of precision.

In a "Report of the Acting Committee of the Edinburgh Homœopathic Dispensary," on the treatment of Asiatic Cholera, from October 8th to October 27th 1848, there is a general statement which gives a total of 77 eases, of which 40 are represented as recovered, 17 to have died, 16 removed, and 4 under treatment. The particulars of 16 eases are given. Thirst is noticed in but a few, though an almost universal accompaniment of the complaint, and no information given as to its having been satisfied or otherwise, and therefore none as to its effects. No uniformity

appears to have been adopted in the exhibitions, but veratrum, arsenic, and nux-vomica were the medicines generally employed, of which the veratrum appears to have been the specially favoured one ; but, in case No. 7, camphor alone seems to have been exhibited, and the patient recovered.

The lancet appears to have been in no case employed ; and, in some, it is difficult to ascribe recovery to the means adopted.

The want of similarity in the treatment observed in Edinburgh to any pursued in either India or England, as given in either the Foreign or Home Reports, strikes me as remarkable, and a discrepancy not to be accounted for by any modification of the disease.

But, though the general treatment of the epidemic in England does not form so great a contrast to that so successfully resorted to in India, by Dr. Burrell, as that exhibited in the Edinburgh Report ; yet, in some very important respects, it has been greatly deviated from. First, as to blood-letting—we are informed that “experience has taught us, that the general result has been less advantageous in Europe than in Asia.” Second, emetics are stated to have, here, a very marked effect in rousing the system from a state of collapse, but no such effect appears ascribed to them in India. Third, opium though admitted to be useful in the early stage, is proscribed at home in that of collapse, but forms, as we have seen, the specific of Dr. Rankeen at Bengal.

Stimulants are admitted, both in India and England, to be of unequivocal advantage, judiciously administered, and with careful reference to the danger of febrile reaction. Of purgatives, calomel and rhubarb are not only admitted to be useful by some, but highly recommended at home and abroad ; while by others it is asserted that, "in the generality of cases, calomel has no effect whatever;" an assertion not very easily to be credited. I am not aware that saline medicines have been exhibited in India ; at home they have been administered both by mouth, and by injection into the veins, and, as stated in the latter case, "been productive of strikingly beneficial results." I confess myself incredulous as to the statement of the benefit ascribed to the saline portion of the medicine ; as also to the idea of deficient alkali in the blood.

External remedies are by some considered highly important ; and the hot-bath, warm air, friction, and hot turpentine to the abdomen are represented as beneficial auxiliaries ; and clysters of various kinds are recommended, without either stating the circumstances, the object, or, if administered, noticing the effect.

Amidst such mystery as to the cause of the disease ; such conflicting views as to the process of its operations ; and such diversified modes adopted in its treatment ; how are we to arrive at a correct conclusion, as to which we should adopt, or which reject ; or, if we cannot be very decisive in our

solution, which among the many presented we should prefer ?

To this I can give but one answer—*that* which in its rationale has common sense for its basis ; or, still better, that which has experience in its favour, and successful practice for its proof. Where, as in the first instance, we could not have the latter, the former affords the safest guide, pursuing it however no further than the principle laid down. On this principle let us consider what, before we had experience to guide us, should have been the therapeutical process adopted.

In the severest form of the disease, the nervous power is stated to have been exhausted instantaneously “like the electric fluid from a Leyden jar,” and to electricity many ascribe the origin of the disease. This would seem to indicate a treatment similar to that adopted in the case of asphyxia produced by lightning ; yet no such mode of treatment, that I am aware of, has been resorted to, even as an experiment, nor would I be very sanguine of its success. In asphyxia resulting from electricity, the blood is uncoagulable ; in Spasmodic Cholera it is resolved into separate elements ; its watery or albuminous portion passing off and leaving behind a lifeless sediment, a *caput mortuum*, deprived of its fluidity and incapable of circulating in the veins.

The asphyxia of Spasmodic Cholera appears to me to have a greater affinity to that produced by inhaling an irrespirable or poisonous gas, and even to be more

nearly allied to apoplexy; and we might reasonably deem a resort to corresponding remedies worthy at least of a trial; and, here, bleeding presents itself as one of the readiest and most decisive. I cannot credit the representation of some that "bleeding *freely* has been often attended with decisive benefit," while the system "has been *weakened* by *more moderate* venesection." Nor does it follow that venesection, whether moderate or free, should tend to further weakness under the greatest apparent prostration of strength. In such circumstances, strength often is not annihilated but overpowered, and venesection will often relieve the system from the weight which loads it, and set its oppressed action free. Moderate venesection may do this in some cases as effectually as the freest, but the degree is the province of the judicious practitioner to determine, who has all the circumstances before him by which he is to decide.

But, theory apart, we have the most conclusive evidence of the beneficial effects of blood-letting, in the practice of Dr. Burrell in India: and, equally conclusive evidence of its injurious effects here, should be adduced to justify our hostility to, or neglect of, a curative process so successful in his hands.

I admit that some who have used the lancet, have expressed themselves dissatisfied, with the result; and one medical gentleman has acknowledged to me that he tried it with many patients and lost them all. If this has been the general experience here, we must treat the statements of Dr. Burrell as fables; but

while these remain as official and authenticated facts, we cannot do so till they are disproved on equally certain and adequate data. I have met with none in which the particular circumstances of its application were detailed; and circumstances in such cases are everything. An attempt to bleed after the characteristic discharges of Spasmodic Cholera have taken place, and the dissolution of the blood has left no living current to course in the veins, must be useless, if not fatal. In such state of the disease a different treatment is clearly indicated; the supply of that *moisture* to the blood of which it stands so much in need, and that *oxygen* which is its living principle. I shall conclude with one observation, that, while no theory should claim attention opposed to well authenticated facts, no alleged facts should be hastily admitted that subvert a rational and consistent theory.

When the attack commences with spasms, the hot bath, opium, and other antispasmodics are sufficiently indicated. I have known the hot bath attended with immediate beneficial effects. But, I do not consider its effects confined to subduing or lessening the rigour of the spasms. I have no doubt it acts beneficially in supplying in some degree that moisture of which the system stands so much in need, and which is so marked a characteristic of the disease; and also that it tends to encourage the action of the liver and kidneys, the offices of which viscera, are so generally affected, and so frequently altogether suspended. That the skin imbibes such moisture freely there can

be no question. It is a well established fact that bathing even in salt water has allayed the thirst experienced at sea, when the supply of fresh has been exhausted.

When the attack commences with vomiting and purging, emetics and mustard poultices have been recommended, the former being stated to have a marked effect in "rousing the system from a state of collapse, and restoring the circulation," but it is admitted, that "such beneficial results are often merely temporary." The choice of the emetic is stated to be of little importance, "provided it be one which excites immediate and full vomiting;" but it does appear to me to be of importance, and though by the Indian Reports, *the anxiety of the patients for cold water* was reluctantly complied with, and accompanied by a caution against its use: and, though by others objected to as increasing the *tendency to vomit*; we have a proof of its beneficial effect *for this very purpose*. We are told "where the vomiting is frequent and violent, it has been found a very useful practice, to keep up this action of the stomach by *copious draughts of cold water*; for, under this simple treatment, *the patient frequently recovers*."

Where the spasms succeed the purging and vomiting, "frictions with camphor liniment and oil of turpentine," are recommended. The warmth of the surface is advised to be kept up by means of "hot bottles, bags filled with warm bran, and frictions with dry powder of ginger." The same authority says,

“the hot bath and the vapour bath are not advisable, while another authority states, the hot bath and hot air with similar frictions to be “powerful auxiliaries.”

Now I do altogether doubt the slightest beneficial effect of dry hot exterior applications, and the hostility evinced by the patients to such applications is quite conclusive, in my mind, of their utter impropriety. The inhaling hot air, I have expressed my belief to be positively injurious. It is not cold the patients complain of, but intolerable heat; and if we could by our applications only distribute the heat, accumulated about the stomach and precordia, there is an abundance of it for the supply of the extremities.

This I believe can only be done by restoring the *oxygenation* of the blood. For this purpose a free supply of *atmospheric air* is important, and I would add the getting the patient to inhale a portion of *pure oxygen gas*, in order if possible by mechanical means to induce its absorption by the lungs, or tend to counteract their restricted capabilities to *oxygenize the blood*.

Where the more specific characteristic of the disease, the exertion by vomiting and purging of matter divested of bile, does not precede the spasmodic symptoms, it is sure to follow them. Whatever, therefore, tends to restore the action of the liver cannot be resorted to prematurely. Calomel in free doses, and especially in unison with antimonials, has been exhibited and recommended by the very first of

the faculty, though by others stated to be "decidedly objectionable."

"The strength of the system," says one writer, "is to be supported by brandy and water, and cordial draughts of sal-volatile, ether, cajuput oil, or essence of peppermint." Another cautions strict limits, to be set to the use of alcohol, lest its effects should prove injurious in the supervening febrile stage; and I have seen myself the propriety of caution in a case where, though the Cholera was subdued by brandy, the fever was rendered fatal. It is a mistake to speak of brandy as keeping up the *strength*: it is merely a temporary *stimulus*.

Such is a sketch drawn from the combined traits of a few of the many who have written on the causes, the symptoms, and the treatment of Spasmodic Cholera. That there is a discrepancy in the opinions entertained as to its cause is not surprising; that the general features of its character are pretty much the same, as noticed by all, must be equally admitted; and that no deficiency of pathological observation can be charged against those who have treated of the subject, beyond some little want of precision as to the order of the symptoms, we willingly allow; but, as respects its treatment, the curative processes adopted appear to have been of the most confused and conflicting character, with but a very few exceptions, devoid of any fixed therapeutical principles, and, I am compelled to add, reproachful to medical science, which we would expect to adopt

some consistent system of treatment, if not to trace up, in the spirit of philosophical inquiry, the latent source of this terrific disease.

As, however, it has been candidly acknowledged by the Faculty, that "all hitherto advanced as to the proximate cause of Cholera is mere conjecture," and that "we are in a state of entire ignorance on the subject," it may be permitted even to one not a practitioner to offer his opinion relative to it.

The proximate cause, then, I consider to be the *insufficient* or *non-oxydation* of the blood itself, occasioned by the breathing a *sub-oxygenated atmosphere*; or by the inhaling some subtle poison with which it is loaded, indigenous to the East, and transmitted to us from thence, on some unascertained principle of progression hitherto unknown; the effect of which poison or other influence is, to suspend the wholesome action of the lungs, or to withhold from them their habitual and necessary requirements, thus palsying, through the medium of the *unoxydized* blood, the action of the brain, and disorganizing again, through its medium, all the animal powers. Or, supposing the *suboxygenated* blood to act immediately on the heart, deprive that viscera of sufficient energy to impel the reluctant blood through the system, thus leaving the deserted extremities a prey to all the suffering of uncontrolled spasmodic action.

With such views as to the cause, I would suggest as to the treatment, what experience seems to have confirmed the propriety of.

First. When the patient is struek down, as if stunned by a blow, and the animal powers suspended : bleeding ; calomel ; electrieity ; stimulants.

Second. When the attack eommenees with spasms : the hot-bath ; opium, and other anti-spasmodies ; frietions with eamphor tea ; oil of turpentine, &c.

Third. When purging and vomiting take plaee : opium ; ealomel in free doses ; stimulants ; emeties ; *mucilaginous drinks* ; and a *plentiful supply of cold water*, as much as ever the patient is willing to receive.

Of course in the exhibition of the several remedies referred to, much must depend, as to the time and eircumstanees under which they are to be exhibited, on the judgment of the attendant physieian ; but there is one matter that I shall dwell on here, though at the risk of being eharged with giving it an importance which it may be deemed not to deserve.

In almost all the notiees I have met with of the disease, the *insatiable thirst of the patients*, and ineessant call for *cold water* is partieularly mentioned.

“The patient,” says Mr. Whyte, “invariably eomplained of great heat at the stomach, and ealled ineessantly for *cold drink*, though warned of the danger attending its use.”

“Where the vomiting,” says another, “is frequent and violent, it has been found a very useful praetice to keep up this aetion by *copious draughts of cold water* ; and, under this simple treatment, the patient frequently reeoovers.”

Dr. O'Shaughnessy says, “The summary of my

experiments may be described as denoting a great but variable deficiency of *water* in the blood," &c.

And Dr. Elliotson sums up with respect to his experience of the Complaint in the following words:—"I really can say nothing satisfactory with regard to the treatment; but I know that patients feel *intense heat* within, and *intense thirst*; and that *they find great comfort from cold drinks*. I understand that, in Vienna, the custom was to allow *ice*, which the patients took with great avidity, and *derived great comfort from it*."

If these testimonies are not conclusive, as to the propriety of supplying cold water copiously to the patient, I know not what evidence would be conclusive. Deficiency of *water* in the blood exists; *intense thirst* exists, in consequence; nature urges the remedy; and we hesitate to employ it, though attended in most cases with success, and in all with comfort; yet in some cases, if not altogether denied, it was given sparingly, and with a warning of "the danger that attended its use."

What the danger was, we are not informed, as I have already, I believe, observed, but the proofs are decisive, that a plentiful supply of water was *often beneficial* and *always grateful* to the patient; that it moderated at least "the great heat of the stomach," and the thirst "of which he invariably complained." But, in my opinion, it did much more. It tended to supply a portion of *oxygen* to the blood, of which it stood so much in need, and thus contributed to re-

store, though often inadequately, the healthy functions of the system, and to equalize its heat, by distributing the accumulation of it, which pressed on the stomach and præcordia; and also afforded some supply of *moisture* to the blood.

It is stated, indeed, by some writers, that the *cold water* increased the *vomiting*, and they urge this as an argument against its use; but we have seen its beneficial effects on the system even *as an emetic*; and the comfort universally attributed to its agency on the patients outweighs every objection to its use that has ever been urged. Besides, it is probable, that though the cold water seemed to increase the vomiting, the benefit sought was not the less derived from it; and, that it was not rejected till it had extracted that *oxygen*, to the want of which we have attributed the source of the disease.

I shall here add one observation. It has been stated repeatedly, and the remark has been emphatically dwelt on, that the attack generally occurs at night, or rather towards morning; and I can myself confirm the circumstance. But has this fact been satisfactorily accounted for, or attempted to be accounted for? I shall offer a conjecture in unison at least with my theory of the complaint.

I have ascribed it to the *deficient oxygenation* of the blood. At night, we shut out as much as practicable the atmospheric air; we shut out the very principle of life; and breathe an atmosphere which every moment is becoming more and more deprived

of it; therefore it is that we are more liable to the attack in the night than in the day; and more at night the nearer it approaches morning. It is very true, under other circumstances this might be of little, though it always must be of some consequence; but in the state of the atmosphere during Spasmodic Cholera, it may be death.

On the question of "contagious or non-contagious" in this disease, I confess my first impression was, that it was not contagious; but circumstances have come under my notice to modify this opinion.

Consistent with the theory I have adopted of a baleful or *sub-oxygenated* atmosphere being the cause of the disease, it does not appear to me unreasonable to attribute to whatever may increase that *sub-oxygenation*, and, in an especial manner to the unventilated apartment of an invalid or others, the office of acting as a medium particularly favourable to the development of the latent poison, or aggravating the consequences of such *sub-oxygenation*, to those exposed to its influence. In this sense I confess my confidence that Spasmodic Cholera is contagious.

But as to such *insufficient oxygenation*, or some baleful influence in the atmosphere, I attribute, under favourable circumstances for its evolution, the disease; so to the same source, which conveys the foreign bane, I look to providing in some measure both the preventative and cure. The FREE ACCESS OF ATMOSPHERIC AIR, both by day and night, I would particularly insist on, in order to compensate by the quantity

for the deteriorated quality of the sub-oxygenated fluid. The exhibition of PURE OXYGEN, in order to supply by meehanical means the defeiency of that of the atmosphere. The liberal supply of COLD WATER, at the option of the patient. Nor, shall I hesitate to sum up with a recommendation, under judicious application, of the COLD BATH.



